## Response to Reviewer #1

We sincerely thank the reviewer for the thorough and insightful evaluation of our manuscript entitled "Experimental Determination of the Global Warming Potential of Carbonyl Fluoride ($COF_2$)". We greatly appreciate the time and effort the reviewer has devoted to carefully reading our paper and for providing detailed and constructive feedback. The comments have been extremely helpful in guiding the ongoing revision process and in identifying areas that require clarification, additional references, and improved presentation of data and uncertainties.

In preparing the revised version, we are (i) integrating multiple references to earlier COF2 atmospheric chemistry studies to better distinguish between established knowledge and new experimental findings, (ii) refining the figures and tables to remove redundancy and ensure numerical consistency, (iii) re-evaluating the radiative efficiency (RE) values using the same methodology as previous studies and comparing them with existing data to verify consistency, (iv) explicitly reporting integrated absorption cross-sections and their associated uncertainties, (v) clarifying the assumptions and limitations related to the well-mixed atmosphere and lifetime correction in GWP estimation, and (vi) removing unrelated $COH_2$ materials from the Supplementary Information. These revisions are intended to enhance the scientific accuracy, transparency, and focus of the manuscript.

We provide detailed responses to each comment below, describing the planned revisions and corresponding improvements. We believe that the revised version, once completed, will more clearly distinguish between prior literature and our experimental contributions while aligning closely with ACP's standards of scientific rigor and clarity. We once again thank the reviewer for their constructive input, which is greatly contributing to strengthening the overall quality of our study.

**Comment & Answer**

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| **Comment #1:**<br>I am not an atmospheric chemist, but it did not take me long to search for and identify several papers on COF2 atmospheric chemistry that appear directly relevant to this work – these include https://doi.org/10.1021/acs.jpca.9b00899 https://doi.org/10.1007/BF00696901, https://doi.org/10.1016/j.cplett.2004.08.022 and https://doi.org/10.1021/j100033a023. Even the COF2 Wikipedia page presents a structure for the COF2 molecule which is very close to the structure presented here as if it is new science. Other databases such as Pubchem and NIST seem to contain well-established information on COF2. |
| **Answer #1:**<br>We thank the reviewer for this valuable and insightful comment. We fully agree that the molecular structure, reaction mechanisms, and atmospheric chemistry of carbonyl fluoride (COF2) have been comprehensively studied in the literature. The works cited by the reviewer—Berasategui et al. (J. Phys. Chem. A, 2019), Francisco (Theor. Chim. Acta, 1987), Uchimaru et al. (Chem. Phys. Lett., 2004), and Zachariah et al. (J. Phys. Chem., 1989)—together with the NIST and PubChem databases, clearly establish the fundamental structure and reactivity of COF2. |

We acknowledge that our previous description of the molecular structure may have given the unintended impression that it represented new findings. In the revised manuscript, this section has been rewritten to clarify that the structural and vibrational information is not newly derived, but is used solely as a reference for interpreting the experimental FTIR spectra and for supporting the analysis of the absorption cross-section (ACS).

To avoid any potential misunderstanding, the DFT calculation section has been removed entirely, and the discussion has been replaced with appropriate references to prior theoretical and experimental studies. This revision ensures that the manuscript clearly builds upon the well-established knowledge of COF2 and focuses on the experimental determination of its ACS, radiative efficiency (RE), and GWP.

**Comment #2-3:**

I do work in the area of radiative transfer. There is existing data of absorption cross-sections (see the supplementary data of Hodnebrog et al. (2020) (already referred to in the paper) and Thornhill et al. https://doi.org/10.1029/2024JD040912, for the source of this data). There is also line-by-line data for the major COF2 bands, readily available on the HITRAN database HITRANonline (it is Gas ID 29). It is essential that the new measurements in this paper are compared with this earlier work and any differences (and possible reasons for the differences) are discussed. The present paper even lacks a discussion of the integrated cross-section, leaving the reader to guess how the measurements compare with the previous literature. I believe this is also essential.

In addition, both Hodnebrog (in their supplementary information) and Thornhill already present the radiative efficiency for a constant COF2 profile (0.123 and 0.126 W/sq.m/ppb respectively) (although Thornhill's reason for focusing on COF2 is not the same as in this paper). This needs to be acknowledged and some comparison of the new value – which is about 17% higher than these two studies – is necessary, with some possible explanations for this quite large difference.

**Answer #2-3:**

We thank the reviewer for these detailed and constructive remarks. After re-examining the integrated absorption cross-section (ACS) and radiative efficiency (RE) results of $COF_2$ from our previously submitted manuscript, we identified several areas for improvement.

In the original submission, the $COF_2$ ACS spectrum was stitched at 1998.803 $cm^{-1}$ by combining two measurements at different concentrations: a high-concentration spectrum (170.65 Torr, 296.79 K) for weakly absorbing regions (to improve SNR) and a low-concentration spectrum (40.24 Torr, 296.77 K) for strongly absorbing regions (to avoid saturation), under the assumption of ACS concentration-independence. Post-review diagnostics revealed that this procedure introduced distortions at low wavenumbers: in the high-concentration portions, several lines were partially saturated, compromising Beer–Lambert linearity and yielding biased band shapes. When integrated against the spectral weighting function for RE, this caused an overestimate of the RE. Consequently, our initially reported stratosphere-adjusted RE ($\approx 0.141$ W $m^{-2}$ $ppb^{-1}$) was about 17 % higher than those in previous studies (Hodnebrog et al., 2020; Thornhill et al., 2024).

To address this issue, we reanalyzed and re-evaluated a single FTIR absorption spectrum (40.24 Torr, 296.77 K, 0.5 $cm^{-1}$ resolution) following the same evaluation procedure introduced in Hodnebrog et al. (2020) and Thornhill et al. (2024). The revised results were compared with these previous studies, and the reasons for the differences were carefully examined, as summarized below.

(1) Integrated ACS Comparison

Thornhill et al. (2024) reported an integrated ACS of $1.55 \times 10^{-17}$ $cm^2$ $molecule^{-1}$ over the 0–3000 $cm^{-1}$ range using the PNNL dataset, while our measurement yielded $1.58 \times 10^{-17}$ $cm^2$ $molecule^{-1}$. The PNNL spectrum covers a broader spectral range (≈550–3000 $cm^{-1}$) than our measurement, including the 550–655 $cm^{-1}$ region where additional absorption features are present, which leads to a slightly larger integrated ACS. However, when comparing only the common range (550–3000 $cm^{-1}$), our integrated value becomes about 2 % higher, likely due to a slightly elevated baseline between 2000 and 3000 $cm^{-1}$. If the PNNL data are restricted to the same 655–3000 $cm^{-1}$ range, the difference increases to approximately 3 %, confirming that the inclusion of the additional low-wavenumber absorption features (550–655 $cm^{-1}$) explains the larger ACS in the PNNL dataset.

For the HITRAN database, only the major absorption bands are represented, and smaller absorption peaks beyond the baseline are not included. As a result, HITRAN-based integrations yield lower Integrated ACS and RE values compared with both our measurements and the results from Hodnebrog et al. and Thornhill et al..

(2) Radiative Efficiency (RE) Comparison

Hodnebrog et al. (2020) and Thornhill et al. (2024) reported RE values of $1.23 \times 10^{-16}$ and $1.26 \times 10^{-16}$ W $m^{-2}$ $ppb^{-1}$, respectively, integrated over 0–3000 $cm^{-1}$. Our re-evaluated result is $1.19 \times 10^{-16}$ W $m^{-2}$ $ppb^{-1}$. As with the integrated ACS, the smaller RE primarily reflects our narrower valid spectral range (655–3000 $cm^{-1}$), which excludes the 550–655 $cm^{-1}$ region containing minor absorption features that contribute to radiative forcing. Consequently, the RE values reported by Hodnebrog and Thornhill are 4 % and 6 % higher, respectively. When our RE integration is restricted to the same 655–3000 $cm^{-1}$ interval, the discrepancy narrows to within approximately 1 %, indicating close agreement.

Taken together, these comparisons demonstrate that our FTIR-based integrated ACS and RE evaluations are fully consistent with previous studies within the limits of experimental and spectral-range uncertainties. The small differences are quantitatively explained by the differences in valid spectral coverage, baseline behavior, and treatment of weak absorption bands (particularly in HITRAN). This confirms that our experimental results are technically sound and reliable within the context of current radiative-transfer methodologies.

**Comment #4:**

There is a lack of discussion of uncertainties, for example on the absorption cross-section and radiative efficiency and, in my view, results are presented with an inappropriate level of precision (e.g., 4 decimal places in the case of RE). Generic estimates of uncertainties (and the source of uncertainties) for this class of gases

are available in the two Hodnebrog Rev. Geophys. papers, and I assume similar generic estimates are available for reaction rates.

**Answer #4:**

We thank the reviewer for highlighting the importance of addressing uncertainties and appropriate numerical precision. We agree that the originally reported values of the absorption cross-section (ACS) and radiative efficiency (RE) were presented with excessive numerical precision (e.g., four decimal places for RE), which may imply a higher level of accuracy than is actually warranted.

In the revised manuscript, we have rounded all reported values to reflect realistic experimental precision and uncertainty propagation. The RE is now reported to three significant figures (e.g., 0.126 W $m^{-2}$ $ppb^{-1}$ instead of 0.1263 W $m^{-2}$ $ppb^{-1}$), consistent with the magnitude of measurement and methodological uncertainty.

Furthermore, a concise discussion of uncertainty sources has been added to Section 3.2. The total uncertainty in the integrated ACS is estimated to be within ±3 %, primarily arising from (i) optical path length tolerance (±1 %), (ii) gas mixture composition (±1–2 %), and (iii) baseline and spectral noise (< 1 %). Propagating these through the RE evaluation gives an overall uncertainty of approximately ±4 %, which is comparable to the uncertainty range reported for similar gases in Hodnebrog et al. (2013, 2020).

This revision aligns both the numerical presentation and the uncertainty treatment of our results with established practices in recent radiative-efficiency studies.

**Comment #5:**

18: "kinetic decay" – in atmospheric science it is more common to call this the e-folding lifetime.

**Answer #5:**

We thank the reviewer for this helpful suggestion. The term "kinetic decay" has been replaced with "e-folding lifetime" to align with standard atmospheric-science terminology. The revised sentence now reads:

"Atmospheric e-folding lifetimes of $COF_2$ were 7.56 h, 36.67 min, and 54.86 min for dry synthetic air ($O_2$-only), high-humidity, and low-humidity conditions, respectively."

**Comment #6:**

*32-34: This definition is incomplete – it is the time-integrated radiative forcing of a 1 kg pulse emission at time t=0 compared to the same mass emission of CO2.

**Answer #6:** We thank the reviewer for this valuable clarification. The definition of GWP has been revised to explicitly describe it as the time-integrated radiative forcing of a 1 kg pulse emission at time t = 0 relative to an equivalent $CO_2$ emission. The revised text now reads:

"GWP quantifies the time-integrated radiative forcing following the emission of a 1 kg pulse of a greenhouse gas at time t = 0, relative to that resulting from an equivalent 1 kg emission of $CO_2$ over the same time horizon."

**Comment #7:**

58-61: Very minor, but I am not sure this text is needed. The ACS is the prime property.

**Answer #7:**

We appreciate the reviewer's suggestion. We agree that the detailed distinction between absorbance and the absorption cross section (ACS) was unnecessary, as the ACS is indeed the primary quantity of interest in this study. Accordingly, the explanatory sentences regarding absorbance have been removed, and the paragraph has been revised to focus directly on the intrinsic molecular nature of the ACS (Lines 58-60).

**Comment #8:**

77: No explanation is given for the lower wavenumber (649 cm-1) lower limit. Presumably determined by the infrared detector?

**Answer #8:**

We thank the reviewer for this comment. The lower limit of 649 $cm^{-1}$ in the earlier version resulted from the spectral range of the experimental FTIR detector. In the revised analysis, the integration was performed over the full range of the stratospheric-adjusted Pinnock curve (0–3000 $cm^{-1}$) at 10 $cm^{-1}$ intervals using the Shine–Myhre LBL dataset. This change ensures full-spectrum consistency, and the results remain unaffected where spectral data are unavailable below 649 $cm^{-1}$, as those regions are simply omitted from the summation. The revised sentence now reads:

"The summation was performed over the range 0–3000 $cm^{-1}$ in 10 $cm^{-1}$ intervals."

**Comment #9:**

*79: No source is given for these CO2 reference values and the specification of a single lifetime for CO2 is inappropriate for reasons explained in all the IPCC WG1 assessments. I recommend that the authors simply adopt and cite the IPCC AR6 AGWP(100) for CO2 which is in the supplementary information for https://doi.org/10.1017/9781009157896.009 at https://www.ipcc.ch/report/ar6/wg1/downloads/report/IPCC_AR6_WGI_Chapter07_SM.pdf

**Answer #9:**

We thank the reviewer for this valuable comment. We agree that using a single lifetime for $CO_2$ is inappropriate and have therefore adopted the 100-year absolute global warming potential ($AGWP_{100}$) reference value from the IPCC AR6 Working Group I Supplementary Material (Chapter 7; https://doi.org/10.1017/9781009157896.009). The $AGWP_{100}$ of $CO_2$ (0.0895 $pW \cdot m^{-2} \cdot yr \cdot kg^{-1}$) is now used as the reference for all GWP calculations, ensuring consistency with the IPCC methodology.

**Comment #10:**

88: “potent” – do you mean in terms of radiative efficiency?

**Answer #10:**

We thank the reviewer for the comment. The term “potent” was revised for clarity. The sentence now reads:

″Due to their long atmospheric lifetimes and strong infrared absorption, these gases exhibit GWPs thousands of times greater than that of $CO_2$.”

**Comment #11:**

91: “emphasize the need” – this isn’t quite accurate. These protocols, amendments and agreements were posed in terms of CO2-equivalence (and hence why GWP-100 is widely used to provide that equivalence). They did not specifically focus on the need to reduce emissions of particular gases, but provided signatories with the option of doing so. It can also be noted that the Kigali Amendment to the Montreal Protocol https://ozone.unep.org/treaties/montreal-protocol/amendments/kigali-amendment-2016-amendment-montreal-protocol-agreed is perhaps also relevant, although not specifically relevant to the gases mentioned on line 85.

**Answer #11:**

We thank the reviewer for the helpful comment. The sentence has been revised to accurately describe that these international frameworks use $CO_2$-equivalent accounting based on $GWP_{100}$ rather than emphasizing reductions of specific gases. The revised text now reads:

″While these species are included in the greenhouse-gas basket under international climate frameworks, the Kyoto Protocol, its Doha Amendment, and the Paris Agreement employ $CO_2$-equivalent accounting based on the $GWP_{100}$, providing Parties with flexibility to manage emissions across different gases according to national strategies.”

**Comment #12:**

98: “moderately toxic” – all references that I looked at seem to say it is highly toxic. I am not in a position to judge.

**Answer #12:**

We thank the reviewer for this careful observation. The term “moderately toxic” was cited directly from Mitsui et al. (2004), where $COF_2$ was described as having toxicity comparable to other fluorinated gases commonly used in semiconductor processes. In that context, the phrase likely referred to its relative handling risk in industrial environments, rather than its absolute toxicity to humans.

However, we acknowledge that $COF_2$ is indeed a highly toxic gas with a molecular structure similar to phosgene, and the use of the term “moderately toxic” could be misleading or open to misinterpretation. As the

toxicity classification is not central to the scientific objectives of this study, we have removed the phrase from the revised manuscript to avoid confusion.

**Comment #13:**

110: The wavenumber range of the measurements needs to be stated. Presumably this is determined by the detector?

**Answer #13:**

We thank the reviewer for this helpful comment. The wavenumber ranges of the FTIR measurements have now been specified in the revised manuscript, as they are indeed determined by the detector type used in each instrument.

In the revised text, the corresponding section now reads:

"The infrared ACS required for RE calculations was measured using a Nicolet iS50 FTIR spectrometer (Thermo Fisher Scientific, USA) equipped with a DTGS detector (spectral range: 4000–650 $cm^{-1}$) and a 2.4 m multipass gas cell (Pike Technologies). Time-resolved monitoring of $COF_2$ decay under different oxidizing conditions was performed using an Arcoptix GASEX OEM FTIR spectrometer (Arcoptix S.A., Switzerland) equipped with a 4-TEC MCT detector (spectral range: 5000–830 $cm^{-1}$), which is a compact and robust module designed for gas-phase spectroscopy."

**Comment #14:**

*157-163: Although quite interesting, no motivation is given for presenting these DFT calculations. Why are they needed, when you have measurements and, in terms of integrated cross-sections, how do they compare with the measurements? If it is to allow attribution of bands to specific modes of vibration, that is interesting, but I think this has already been established in the earlier literature – for example, see the papers that are referred to by the HITRAN database for COF2. So, again, more references to the older literature are necessary and the text can be shortened so that it doesn't appear as if the results presented here are original.

**Answer #14:**

We thank the reviewer for this helpful comment. We fully agree that the DFT calculations were not essential for this paper. Our original intention was to show how the molecular structure of $COF_2$ relates to its infrared absorption features and atmospheric behavior in a single discussion. However, we understand that this may have caused confusion, as these structural details are already well established in previous studies. In the revised manuscript, we have removed the DFT calculation results and instead referred to the existing literature and the HITRAN database for the vibrational mode information. This makes it clear that our focus is on the experimental FTIR measurements and the evaluation of ACS, RE, and GWP of $COF_2$.

**Comment #15:**

*168-181: Again, this section is presented without any references to earlier work, implying that this is new knowledge. As noted above, the molecular structure presented here is very little different to the COF2 entry on Wikipedia. I am not sure why the authors choose to compare with formaldehyde. This seems to lengthen the paper unnecessarily and can be removed.

**Answer #15:**

We thank the reviewer for this valuable comment and fully agree with the observation. The discussion comparing $COF_2$ with formaldehyde and the associated structural description does not add essential scientific value to this study. In the revised manuscript, this section has been removed to maintain focus on the experimental results and to avoid any implication that the structural information represents new findings.

**Comment #16:**

Figure 2a: Although I believe Figure 2a is not necessary, note that the angles in the figure and in the caption are slightly different. There is also no need, in my view, to repeat information that is in the figure in the caption.

**Answer #16:**

We thank the reviewer for this careful observation and fully agree with the comment. Figure 2a and its caption are not essential to the main discussion and may cause unnecessary repetition. In the revised manuscript, Figure 2a has been removed to improve clarity and conciseness.

**Comment #17:**

*Figures 2b and 3b: the authors need to revisit how that they handle the low wavenumber noise in the FTIR spectra. The fact that the noise sends the ACS negative is completely unphysical but it is not recommended that these are set to zero whilst retaining positive noise, as this introduces a bias. Many studies simply exclude regions between bands (i.e. set the ACS to zero) where the signal to noise ratio is too low.

**Answer #17:**

We thank the reviewer for this valuable and technically insightful comment. We fully agree that the low-wavenumber noise in the FTIR spectra should be handled carefully to avoid unphysical negative absorption cross-section (ACS) values and potential bias.

In the revised analysis, the FTIR spectra were averaged onto a 10 $cm^{-1}$ grid prior to ACS calculation. This averaging effectively suppresses random noise, removes negative excursions in low–signal-to-noise regions, and eliminates bias introduced by selectively setting only negative values to zero.

Furthermore, this 10 $cm^{-1}$ grid spacing is directly consistent with the spectral resolution of the Narrow Band Model (NBM) used in the radiative efficiency (RE) calculation. Therefore, the averaged ACS data can be applied in a one-to-one correspondence with the NBM spectral grid, ensuring both numerical consistency and physical validity in the RE evaluation.

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| **Comment #18:**<br>184-189: Some of this discussion repeats information provided already in Section 2. |
| **Answer #18:**<br>We agree with the reviewer's observation that the discussion in lines 184–189 partially overlaps with the content already presented in Section 2. In the revised manuscript, the redundant sentences have been removed to avoid repetition and improve the overall conciseness of the text. |

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| **Comment #19:**<br>*190-204: Again results are presented without a single reference to previous work in this area. See my comment on 157-163 for where some of earlier papers can be found. |
| **Answer #19:**<br>We thank the reviewer for this helpful comment. We agree that this section lacked appropriate references to prior studies. In the revised manuscript, we have added citations to the earlier works mentioned in Comment #14, as suggested by the reviewer, to properly acknowledge existing research in this area and provide adequate scientific context. |

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| **Comment #20:**<br>*183: This section needs to report the integrated absorption cross-section and to discuss uncertainties in the final value. |
| **Answer #20:**<br>We thank the reviewer for this valuable comment. As discussed in Response #2–3, the integrated absorption cross-section (ACS) has been recalculated and compared with previous studies. In the revised manuscript, we have additionally included a discussion of the uncertainty in the final integrated ACS value, considering key experimental factors such as path length tolerance, gas concentration accuracy, and baseline stability. This addition ensures a more complete and transparent evaluation of the measured ACS. |

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| **Comment #21:**<br>*217: Is 4 decimal places justified? How does this value compare with previous literature and what are possible reasons for the difference? And what is the estimated uncertainty in the IAC and RE. |
| **Answer #21:**<br>We thank the reviewer for this insightful comment. We acknowledge that the number of decimal places in the reported RE value was not carefully considered in the original submission. The values were presented with four decimal places purely for visual clarity, without a proper assessment of significant figures or measurement uncertainty. |

We fully agree with the reviewer that establishing appropriate significant figures and reporting uncertainties are essential for meaningful comparison with previous studies. In the revised manuscript, we will adopt the uncertainty treatment and numerical precision approach recommended in the references cited by the reviewer (e.g., Hodnebrog et al., Rev. Geophys., 2013; 2020) to ensure consistent and scientifically justified reporting of both the integrated ACS and RE values.

**Comment #22:**

*218: "measurable contribution to RF". Without estimates or measurements of the abundance of C2FO it is not possible to make this statement, and I doubt if the RF can be measured as it is so small. The statement should either be better justified, or the authors should stick to RE, which is what they calculate and present.

**Answer #22:**

We thank the reviewer for this helpful observation. We agree that the statement regarding a "measurable contribution to RF" was not appropriate, as the present study does not provide any estimates or measurements of the atmospheric abundance of $COF_2$. We also acknowledge that, given the expected low concentrations, its direct radiative forcing would indeed be extremely small and difficult to quantify.

In the revised manuscript, this sentence has been modified to focus only on the radiative efficiency (RE), which is the quantity actually derived and analyzed in this work, and any implication of measurable RF has been removed.

**Comment #23:**

*221-273: Again there is not a single reference to the older literature in the whole of this section, and so the impression is given that this is all new. I believe that the reactions given in Scheme 1 are already well established.

**Answer #23:**

We thank the reviewer for pointing out the lack of references in this section. Upon review, we fully agree that additional citations are needed to contextualize the proposed $COF_2$ degradation pathways. In the revised manuscript, we have added appropriate references to previous studies on the oxidation and hydrolysis reactions of carbonyl fluoride and related fluorinated species.

These references clarify that while the general removal mechanisms of $COF_2$ have been discussed in earlier work, our experiments specifically distinguish between $O_2$-mediated oxidation and $H_2O$-driven hydrolysis under controlled laboratory conditions, providing quantitative kinetic comparison under dry and humid atmospheres.

**Comment #24:**

226: "negligible". I am not sure what this means. Its presence is very clear in Figure 4a, but the reader is not told this.

**Answer #24:**

We thank the reviewer for this careful observation. We agree that the use of "negligible" was misleading. As correctly noted, small water-related absorption features can indeed be seen in Figure 4a. These signals likely originate from trace amounts of residual moisture in the gas cell. Although the nominally "dry" synthetic air mixture contained less than 2 ppm of water vapor, the cell was filled to near atmospheric pressure, meaning that the total number of $H_2O$ molecules present was still sufficient to produce detectable absorption bands.

We appreciate the reviewer for drawing our attention to this point. In the revised manuscript, we will clarify this explanation in both the text and the caption of Figure 4a to ensure that readers correctly interpret the presence of these minor water features under "dry" experimental conditions.

**Comment #25:**

*Table 2: Some values are quoted to 6 significant figures (see also other tables in the paper) without any accompanying uncertainty estimate.

**Answer #25**:

We thank the reviewer for this helpful observation. We fully agree that the numerical precision reported in Table 2 (and other tables) is not appropriate without accompanying uncertainty estimates. In the revised manuscript, the number of significant figures will be determined based on the actual measurement precision, and corresponding uncertainty values will be added with reference to previous studies and established literature in this field.

**Comment #26:**

*225: More care is needed about such statements as "typical outdoor conditions". While they may be correct for near-surface conditions where the measurements were made, they may not be appropriate elsewhere. Despite the short lifetimes found in this study, it may be possible for convection to transport some surface emitted C2FO to reach high altitudes and hence lower humidities. Similarly, emissions in high latitudes may not experience such moist conditions. The authors could simply point out that these lifetimes are appropriate to the conditions in which measurements are made, but they may not be appropriate for all locations.

**Answer #26:**

We thank the reviewer for this insightful comment and fully agree that our statement should be more carefully qualified. In the revised manuscript, we will clarify that the reported lifetimes correspond specifically to near-surface outdoor conditions under which the measurements were conducted, representing typical ground-level environments. We will also note that these lifetimes may vary under different atmospheric conditions—such as at higher altitudes, in drier or colder regions, or across different latitudes and seasons—where humidity and temperature differ significantly from the conditions of this study.

**Comment #27:**

*275: The reader should be reminded that these estimates of RE and GWP use the assumption that COF2 is well-mixed, which is highly unlikely given the short lifetime. Although not fully appropriate to the destruction processes for COF2, the Hodnebrog papers present simple methods to adjust the RE (and hence GWP) for gas lifetime. Consequently, the values presented here are likely overestimates. In addition, it should be noted that the GWP for very short lived species is dependent on both the location and time of year of the emissions. A short statement on this may be useful.

**Answer #27:**

We thank the reviewer for this valuable comment and fully agree with the assessment. In the revised manuscript, we will apply the lifetime-based correction approach for RE (and consequently GWP) as described in the studies by Hodnebrog et al. (2020) to account for the likely overestimation arising from the well-mixed assumption. We will also include a short statement noting that the GWP of very short-lived species can vary depending on the emission location and time of year.

**Comment #28:**

299-291: This statement is not correct for the AGWP of CO2.

**Answer #28:**

We agree with the reviewer. The explanation of AGWP for $CO_2$ will be corrected in the revised manuscript. We will clearly describe the definitions of AGWP for both $CO_2$ and $COF_2$ based on the IPCC methodology and adopt the AGWP(100) reference value for $CO_2$ from the IPCC AR6 WGI Supplementary Material.

**Comment #29:**

Supplementary Material: I feel that all the information concerning COH2 should be removed, as its relevance to this study is never established. It is also presented without any reference to the older literature which is extensive for this molecule.

**Answer #29:**

We agree with the reviewer's comment. In the revised manuscript, all information related to $COH_2$ will be removed from the Supplementary Material, as it is not directly relevant to the present study and is already well established in the literature.